

the red spot formerly was is now very white; it passed over the central meridian of the planet this morning at 4h. 36m. (M.T. at Palermo), which gives for this place the Jovicentric longitude 63° , plainly corresponding to the longitude that Mr. Marth assigned to the red spot at present, if visible. This proves that the neighbourhood of the red spot had followed the particular motion of the spot itself. This place is well characterised by the permanent depression in the great reddish band of the planet.

A. RICCO

Royal Observatory, Palermo, September 10

"Elevation and Subsidence"

MR. O. FISHER has been so good as to offer a reply to my "remark with a query," his answer being (allowing for an obvious printer's error) that it is "an open question whether the melting temperature of rocky matter is, or is not, raised by pressure."

I cannot for a moment pretend to the same familiarity with the results either of experiment or of calculation as is doubtless possessed by Mr. Fisher. I only claim to speak as representing the class whose knowledge on these subjects is essentially second-hand; but, speaking as such, I think that Mr. Fisher's reply will not generally be regarded as satisfactory. I should, therefore, like to repeat my question with a little extension:—

1. Do not the "rigidity" calculations incontestably show that the earth is extremely rigid, *i.e.* solid? Are not, therefore, all theories which disregard this result (such as that the nucleus may be above its own critical temperature) put out of count?

2. Are not the phenomena of metamorphic and hypogene rocks on too large a scale to be accounted for by heat of merely local origin, whether produced by chemical or mechanical action, such as has been suggested in connection with volcanoes?

3. Do not all reasonable views of the origin of the earth, *i.e.* any form of the nebular hypothesis, point to the same conclusion as (2), viz. that the earth's heat is the residuum of a much greater amount formerly possessed, and not yet entirely lost by radiation?

4. Does not (3), taken in connection with the known laws of conduction, involve a continuous increase of temperature, whether rapid or slow, as we descend below the surface?

5. Although we may have no *direct* evidence as to the "temperature at depths bearing considerable ratios to the radius," is there not ample evidence that at comparatively insignificant depths the temperature is such as would melt not only "rocky matter," but far more refractory substances, if there were no counteracting influence? Even allowing a very slow increase, provided the increase is always positive, as 4 points out, should we not sooner or later almost certainly reach the melting temperature of the most refractory substances with which we are acquainted?

6. Can we then escape the conclusion, either that the nucleus consists of matter of a totally different kind from anything with which we are familiar, or that pressure raises its melting temperature? But does not every fact bearing on the question discredit the former hypothesis?

7. Should we not then accept the view that pressure does raise the melting-point of nucleus stuff, at least as a working hypothesis, only to be overthrown by direct evidence to the contrary, if direct evidence on the subject is ever forthcoming?

Trinity College, Cambridge

F. YOUNG

IN a paper I read before a full meeting of the Geological Association on March 2 last, of which a brief notice is given in *NATURE*, vol. xxvii. p. 523, I discussed the probability of subsidence of land, in certain cases, being due to *loading* by local accumulations of terrestrial matter acting upon a deflectible crust supported upon a viscous interior. The greatest effects, I imagined, from this cause, were due to local accumulations of ice past and present, particularly about the poles of the earth; but that secondary and important effects were due to the weight of accumulations of solid mineral matter from denudations being carried by oceanic currents and winds, from coral deposition, and the reaction of volcanic outflows. One illustration I proposed was that the sinking of the coast of Greenland was probably due to the weight of inland accumulation of ice, which proposition I thought was original, but Mr. Gardner (*NATURE*, vol. xxviii. p. 324) says—"It has often been supposed that the sinking of the coast of Greenland is similarly due to its icecap." I should

feel obliged if Mr. Gardner would point out references where this has been proposed, as I thought I had read the literature of the subject, and I fear that this part of my paper is less original than I assumed.

W. F. STANLEY

THAT there is a connection between sedimentation and subsidence on the one hand and between denudation and elevation on the other is a fact now admitted by most geologists. The real question to be answered, however, is:—Are these directly connected as cause and effect? or are they simply concomitant effects of the same cause? If the first be true, we should expect cause and effect to vary together, that is, that subsidence should keep an even pace with sedimentation. That this has not been exceptionally the case is proved by the sections of the carboniferous system in the central valley of Scotland, where the facts point to a continuous subsidence, accompanied by a very irregular sedimentation, with the result that now subsidence gained on sedimentation, now sedimentation on subsidence. Again, once the process commenced—and it is not very evident how on an originally even surface it could have commenced at all—we should expect it to be continuous. Sedimentation causes subsidence, subsidence gives rise to fresh sedimentation, and that again to renewed subsidence, and so on and on. Consequently we should expect that when once an area of sedimentation and subsidence was formed, it would continue an area of sedimentation and subsidence through all geological time.

It appears rather, I think, that the connection between them arises from their being concomitant effects of lateral pressure in the earth's crust (for notwithstanding the Rev. O. Fisher's masterly exposition of the inadequacy of this cause to produce the observed inequalities of the earth's surface, I still believe that, with the exception of the ocean basins, which must be otherwise accounted for, it is quite competent to account for the facts). We may suppose the action to take place so:—

A certain portion of the earth's crust is first thickened and strengthened by volcanic outburst or other accumulation on the surface. This part, when the tangential thrust comes, offers, by reason of its increased weight and thickness, a greater resistance to the elevating force than the parts around, and as a consequence these are raised around the thickened part, while it is at the same time depressed in a corresponding degree; in other words it becomes the centre of a syncline, while the strata around are raised into anticlines. Depression naturally leads to sedimentation, and this still more thickens the part, and enables it to offer greater resistance to the tangential thrust, with the result that it continues to be depressed as the strata around are elevated. The converse is also true. Denudation means the thinning and consequent weakening of the crust, and hence when the thrust comes the denuded part is the more likely to be elevated into the anticline.

This theory provides for the cessation of the phenomena, since the tension of the crust is after a time relieved. It also accounts for the fact that strata around volcanoes and volcanic necks, as also along the base of mountain chains, so frequently appear to dip below them. The rate of subsidence, too, would vary with the intensity of the exciting force, though the consequent sedimentation need not vary with it in the same absolute degree.

Perth, September 3

WILLIAM MACKIE

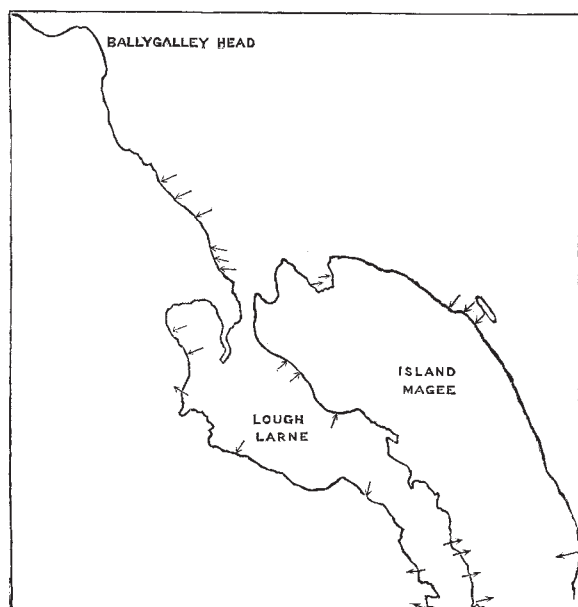
MY article on elevation and subsidence has provoked considerable and, on the whole, friendly criticism, a so far satisfactory result, though but few points have been raised requiring reply. Dr. Ricketts objects, and very properly, that I have not alluded to his many writings on the subject; and to this I can only plead want of space, that I have not entered at all into its already voluminous bibliography, and that my article was written and in type before his recent contributions to the *Geological Magazine* had appeared. Beyond this I had sufficiently indicated that there were many observers in the field, and every geologist must be aware that the subject has for a long while past excited attention not only in England but in France and America.

The fundamental error in my article is pointed out by the Rev. Mr. Fisher and by Mr. Young, and the assumption that inert pressure induces heat must be abandoned. As I had read the "Physics of the Earth's Crust," I expected that this would be challenged, but I let it stand, as the fallacy has been shared by a large number of geologists, comprising some of the most distinguished, and has even escaped the correction of physicists. But this rectification, while very important, by no means affects the results, and on the contrary facilitates an appreciation of the

causes of movements of the earth's crust; for if the fluid or viscous layer is chiefly due to internal heat and the relaxation of pressure near the surface, it may exist much nearer to our feet than could otherwise be admitted.

One of the gravest difficulties that the theory that added weight produces subsidence by acting on a fluid layer has had to contend with has been the great depth at which this fluid layer has had to be placed. It has always seemed to me next to impossible that liquid lava could well up from any such depths as those assigned to the viscous layer, or that a solid crust of so great a thickness should be sensitive to, as it is now shown to be, and rise and fall under, barometric changes. In acknowledging Mr. Fisher's letter and thanking him, I feel I am ungrateful in questioning that part of his work which interposes barriers which would break up the continuity of the viscous layer; I allude to his theory of "the roots of mountains." There does seem to me to be little fact in support of so startling a proposition, and I think the existence of volcanic vents, scattered through and in the midst of some of the highest chains, renders its acceptance difficult.

Mr. Murray restates his theory of the formation of coral atolls and reefs in the clearest manner, but I do not see that he explains any fact left unexplained by Darwin, or exposes any flaw in Darwin's reasoning. These masses of coral may have been continuously forming throughout even successive geological



Sheet 21. Geological Survey of Ireland, Antrim Coast, facing north-east.

periods, and their thickness is perhaps not exceptionally remarkable relative to that of slowly deposited oceanic sediments. There is no evidence that atolls are mere incrustations of volcanic craters, and it seems to me difficult to imagine so great a number of craters at the same level so completely masked. There are volcanic isles in abundance outside coral areas, but none I think, or few, of the form of a coral atoll. After all, Mr. Murray only shows that a second explanation is possible, though I still prefer the first.

I regret, being from home, that I am unable to answer Mr. Stanley. I may have alluded to the sinking of Greenland myself, and if I did not it was because the illustration was too familiar and self-evident. The sinking on the Greenland coast is not, I have understood, universal.

I still think it would render a service to science if readers of NATURE residing on sea-coasts would furnish authentic examples of elevation or subsidence or of waste. The magnificent Antrim coast, which I have recently visited, furnishes examples of subsidence among most unyielding rocks. The cliffs on the mainland are capped with basalt and dip inland, yet the basalt reappears in the Skerries out to sea with the same dip and at a much lower level. The same correspondence in stratification is seen between the mainland and Rathlin, but also with a great difference in elevation. The dip inland in all cases on this coast

should bring up much older rocks out to sea, unless we are prepared to admit a fault running parallel to the coast, and following its sinuosities, and at right angles to the general lines of faulting.

The way in which all the strata forming the cliffs along the Antrim coast dip inland is very remarkable. The accompanying tracing from the Geological Survey Map is of a particularly indented coast-line, and the arrows show that the dip is everywhere away from the sea, irrespective of any general strike. In fact the general strike must often be the reverse of that shown on the coast for the same strata crop out at much higher levels on the hills farther inland. I recollect that most cliffs that I have examined, particularly in Hampshire, dip away from the sea. It would appear that the removal of weight along a cliff line causes a local elevation, which gives a cant inward, whilst subsidence takes place under sediment farther out to sea. This seems to explain the observed facts connected with marine denudation; but I must take a future opportunity of entering more thoroughly into this part of the question.

Glasgow, September 12

J. STARKIE GARDNER

"Zoology at the Fisheries Exhibition"

LETTERS have been published in NATURE of August 9 and 16 (pp. 334 and 366) by Mr. Bryce-Wright of Regent Street and Prof. Honeyman of Canada, calling in question the accuracy of statements made in an article in NATURE (vol. xxviii. p. 289) which were condemnatory of exhibits for which these two gentlemen are respectively responsible. It is natural that they should seek to remove the unfavourable impression which the statements in question were intended to convey: they seem, however, to have been unacquainted with the complete character of the information upon which the statements were based. Mr. Bryce-Wright states that it is not the fact that some of the corals exhibited in Lady Brassey's case belong to him. Nevertheless it is the fact that when the jury of Class V. asked Mr. Bryce-Wright to point out the corals entered in the official catalogue under his name, No. "8136," he informed them that the corals so entered were in the same case with Lady Brassey's corals, and formed part of that collection. It is also the fact that in the opinion of experts the names attached by Mr. Bryce-Wright to many of these corals are incorrect; and as to his assertion that these specimens have been compared with those in the British Museum and with those obtained during the Challenger Expedition, it is a fact that neither the one series nor the other has been accessible for such purposes for some considerable time, and I have reason to believe that no qualified zoologist has made a comparison of the corals exhibited by Lady Brassey and Mr. Bryce-Wright with any collection at all.

The letter of Prof. Honeyman in reference to the naming and state of preservation of the Collection in the Canadian Department, for which he is responsible, is misleading. The discreditable state of that collection, to which a passing allusion only was made in NATURE, has been remedied in one or two instances since the visit of the jury of Class V. Should there be any doubt as to the justice of the opinion expressed in the article in NATURE, I would simply ask Prof. Honeyman whether he would have any objection to allowing the matter to be decided by reference to the report of the jury of Class V., of which he was a member. I should be surprised (and so I think would he) were the report of that jury, when published, found to be at variance with the opinion expressed in the article in NATURE. Prof. Honeyman's statement that the specimen of *Cryptochiton Stelleri* is properly exhibited in a convenient glass jar and labelled inside and out, is calculated to mislead. When first exhibited it was not labelled with any name; subsequently it was labelled with the name of a genus of Holothurians, "Psolus." After the visit of the jury of Class V., probably as the result of information imparted by some of the eminent zoologists who served on that jury, it was labelled with its proper name. Without citing details, I shall simply state that there are (or were when the article in NATURE was written) far more serious blunders in the identification of specimens and worse instances of bad preservation in the Canadian collection of Invertebrata than those to which special allusion has been made.

THE WRITER OF THE ARTICLE

A Complete Solar Rainbow

MR. D. MORRIS, in his account of this rainbow (p. 436) appears to have fallen into a mistake in stating that its inner dia-